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Trip to California, Ann Arbor and Pittsburgh...

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Trip to California, Ann Arbor, and Pittsburgh, May 31 to June 18, 1949

J. J. Stoker

Tuesday, May 31: I arrived in Berkeley in the evening and went to Hans Lewy's (Professor of Mathematics, University of California). Started talking about water wave problems and making plans for my stay. Lewy has made considerable progress with certain mathematical questions which have arisen from the study of water wave problems. This will be discussed in more detail later.

Wednesday, June 1: Today I went to see J. W. Johnson (Associate Professor, Mechanical Engineering, University of California), one of Dean O'Brien's associates, to persuade him to test Fritz John's theory of floating bodies experimentally. This was one of the main objects of my trip. Johnson has agreed to do this, and I have agreed to give him a concise summary of the formulas, assumptions, and basic conclusions. (Since returning home I have received a letter from Johnson, together with a set of proposals for performing experiments of various kinds, a good many of which amount to verifying various theories which we have proposed, especially Fritz John's work on floating bodies, the work on breaking of waves, and Dressler's work on roll waves.) Johnson took me to look at their wave channel so that I might gain an idea of its dimensions, its possibilities for producing waves of various frequencies and wave length, etc., so that I might suggest experiments to be performed rather concretely, even possibly suggesting dimensions, wave lengths, etc. I am glad that this will be done because I consider Fritz John's work may possibly lead to useful practical results for building breakwaters and other purposes.

Johnson then took me for a tour of his other installations, a good many of which I had seen before. He has tanks for studying transport of sand along the beach. A. Einstein, who is now there instead of at California Institute of Technology, has built another ripple tank. Finally, I spoke briefly with Schaaf and Chambre, and saw their equipment. I will see more of Schaaf later on.

In the afternoon I talked at some length with C. B. Morrey (Professor of Mathematics) about Nirenberg's work in differential geometry, which leans essentially on a certain theorem concerning

partial differential equations, the proof of which was obtained largely by using certain tricks of Morrey's.

In the evening I was invited to Lewy's for dinner, together with G. C. Evans, Schaaf, Kelly, and their wives.

Thursday, June 2: The morning was spent talking with Lewy about his recent work. He has developed a general theory regarding the functional character of solutions of potential problems in a half plane when mixed boundary conditions are given which in turn are different for $x > 0$ than for $x < 0$. This was suggested by certain water wave problems, in particular the dock problem. Lewy has now developed a very extensive and difficult theory, too complex to be summarized here. However, it can be said that the basic idea is to operate with a differential difference equation for which he has an invariant integral. This idea in turn makes it possible to discuss the behavior of the solution near the origin and at ∞. (On my return, A. Peters tells me that he has been working on somewhat similar problems, and also makes use of a differential difference equation. Peters representations are, however, different from Lewy's, as far as I can see, and explicit.)

Friday, June 3: I spent the morning writing letters and preparing a talk on water waves which I am to give this afternoon. I was invited to lunch with Dean O'Brien, Johnson, Einstein, and Folsom. They asked me many questions about Fritz John's floating bodies theory and Dressler's work on roll waves. Dressler's work always arouses curiosity.

After lunch I gave a one-hour talk called "Recent Progress in the Theory of Water Waves" to a mixed group of engineers (mainly O'Brien and his people) and mathematicians.

In the later afternoon I went with Schaaf and Chambre to see their wind tunnel for rarefied gases in operation. Since others better equipped with special knowledge than I have doubtlessly described this, I shall not do so. However, I had a good impression of the project. They seem to work in a decisive and business-like way.

Saturday, June 4: Today was spent with Lewy and his wife visiting G. C. Evans at his farm on Mt. Diabolo. After lunch we drove to the summit of the mountain. In the evening there was a dinner and party at

Morrey's house, where I met most of the members of the Mathematics Department, a good many of whom I had met at Aberdeen during the war.

Sunday, June 5: Today I left Berkeley for Los Angeles by plane, and stayed in Los Angeles over night.

Monday, June 6: In the morning I left by train for La Jolla to visit people at the Scripps Institute of Oceanography. I have been working for several years with Dr. Walter Munk and Mr. Robert Arthur on various problems concerning water waves. It had been two years since I had visited them and I was curious to learn what they had been doing and to tell them what I had been interested in. I told them about Fritz John's work on floating bodies, Peters' work on ship waves, Isaacson's work on waves on sloping beaches, and Dressler's work on roll waves. Munk then told me of his work during the last year, which concerns general circulation of currents in the oceans due to wind action and the very important effect of the Coriolis term arising from the earth's rotation, which has the effect of pushing currents against the east coasts of the world's land masses. This is a very interesting affair. The basic theory does not please me too well since it is formulated in terms of Prandtl's concept of mixing lengths in order to take care of turbulence. However, once one accepts the theory the problem becomes very interesting from a mathematical point of view. A stream function is introduced, and it satisfies the bi-harmonic equation with an additional term in the first derivative and a non-homogeneous term on the right hand side due to friction forces caused by the wind. The first derivative term arises from the above-mentioned Coriolis acceleration term. This latter term, it happens, has a very large coefficient, so that one has a boundary layer effect. In effect the solution in the interior of the domain is given very closely by neglecting the 4th derivative terms, the latter playing a role only near the boundary, that is, near the shore line. The result is very rapid changes in the currents in a narrow strip near all east coasts of the land masses. I find this a very amusing result. Furthermore, the differential equations and the boundary conditions are practically those for the bending of the clamped thin elastic plate.

Munk wants me to discuss methods of solving the boundary value problem with him. I promised to study his paper and talk it over with him tomorrow.

After our discussion, Arthur, Munk and I went swimming in the surf, which was fine, as usual in La Jolla. Afterwards they took me to my hotel, which proved to be very comfortable.

Tuesday, June 7: I spent the morning reading Munk's paper on ocean currents and in writing letters.

After lunch Munk and I went to work on his paper. Munk asked me first about the possibility of using the method of finite differences. I told him I thought it the best method to use in this case. We have used this method in solving the plate equation (essentially the same as his equation). However, Munk had somewhat naive notions about the amount of labor that would be involved, and about the difficulties there are in solving large numbers of linear equations. Of course, if he could interest some of the people with computing machines in his problem that might be a solution to this difficulty; otherwise I recommended that he use the relaxation methods and explained to him what those are. I also told him that there was no escaping a certain amount of drudgery in order to gain the necessary experience, but that the problem could certainly be solved in this fashion. We then went on to formulate quite specifically some special simplified problems which might be considered as fair approximations to the actual situation of the land masses and oceans of the world relative to each other. One particularly interesting case, it seems, is that of the currents around the Antarctic. So far Munk's crude approximations have not conformed to the facts in this case, I think because he did formulate the problem too crudely. However, when he replaced the Atlantic Ocean and the East Coast of North America by a rectangle with one side corresponding to the East Coast, he found good qualitative agreement with observations on the Gulf Stream, and even fairly good quantitative agreement. For example, it amused me particularly that the solution had a vortex point at what would correspond roughly to the location of the Sargasso Sea.

Munk asked whether it would be possible for him to come to study with us for a time, with the object of getting some help with his problems. Of course I told him that was perfectly possible.

After this long discussion we took a swim once more.

Wednesday, June 8: The morning was spent writing my diary and some letters. I then left by train for Los Angeles, where H. Busemann (Professor of Mathematics, University of Southern California) met me and took me to his home One of my objects in seeing Busemann was to tell him about the work of two of our graduate students this year in differential geometry. As I had anticipated, he was not so interested in Nirenberg's work on the existence of closed convex surfaces with a given metric, since Nirenberg assumes various differentiability conditions. He was more interested in Van Heijenoort's work in which he deals with surfaces that are locally convex but not differentiable. He told me that Van Heijenoort's theorem had been anticipated by A. D. Alexandroff in a recent book which I had never heard of. (Since returning, Van Heijenoort has found the book but tells me that he cannot find his theorem stated there. However, the book is in Russian, and it may be that Van Heijenoort missed it.)

Thursday, June 9: Busemann was kind enough to drive me to Pasadena in the morning. We met DePrima (Assistant Professor, California Institute of Technology). We talked together for a time and then had lunch together. In the afternoon I had a long talk with Knapp, V. A. Vanoni, and John H. Carr -- all interested in hydraulics. We talked for more than two hours about our work and its possible applications. Knapp complained about the gap between the theory and the applications, implying, of course, that the mathematicians should go all the way to meet the engineers. I told him I thought it was time that the engineers made a few moves in our direction too. However, it was all very amicable and good-natured. Again I told the long story about Fritz John's floating bodies theory, which interested them at least as much as it did the people in Berkeley. They also found the roll waves business amusing. At present they are just writing their report on the results of their model test of the breakwater and harbor designs for Guam Harbor. This model was on a very large scale. They asked me, "Could you compute the waves in that harbor?" I told them I thought it possible, though difficult, the principal difficulty being that one is not quite

sure of the boundary conditions. If one knew how much energy is absorbed by the incoming waves on various parts of the shore, one could solve the problem numerically by finite differences, though not without considerable labor.

One extremely interesting thing they told me about their experiments on the model of Guam Harbor was the following: if one observes the wave amplitudes at a fixed point in the harbor, the graph of the wave amplitudes shows an effect like beats. The beats have a very long period. Beats could be understood easily enough if the wave generating apparatus caused two frequencies which differ slightly, but this they feel is impossible. Another possible explanation would be that the effect is a sub-harmonic of high order, caused by non-linearity in the system. However, that also does not seem very likely to me, because the non-linearity in the system is certainly slight, and the only cases I know of in which sub-harmonics of the order of the 200th occur are systems in which the non-linearity dominates the phenomena. Still another possibility, and perhaps the most likely one, is that the free oscillation of the harbor basin is excited in some way. I asked them whether the period observed was in the right range for this, and they said that it was. However, this oscillation persists for a long time and one wonders why the free oscillation, even if it did occur, would not be damped out.

We talked next at some length about the roll waves; in particular, they were much exercised about the fact that Dressler's solutions are not unique in the sense that after fixing the average discharge rate, the slope of the channel, and the roughness coefficient, one can still prescribe either the period or the wave length arbitrarily. They may want to perform some experiments to test the theory.

In the evening I had dinner with DePrima, Lagerstrom, and three of the younger people in the Aeronautics Department.

<u>Friday, June 10:</u> Got up early in order to go to Azusa with Carr to look once more at the model of Guam Harbor, some of their wave records, and to talk more with Carr, who has taken Wagner's place as Head of the project. As they told me yesterday, the wave records do look just like <u>beats</u>, and maybe they are just that.

Carr showed me some papers about the breaking of waves written by Schiff (Stanford University) and by G. I. Taylor, which I had not heard of before. The idea is to cause breaking by injecting air into the water. The break is caused by setting up a water current in the direction against the incoming waves. The hope is that this might lead to a practical means of reducing wave amplitudes by causing them to break earlier. As one might have expected, it was not too good an idea since it takes too much power, except for short waves. More interesting to me was the theoretical basis. Oddly enough, Taylor uses a purely linear theory. The object of his calculations is to see when the wave lengths become no longer real. I feel that breaking is essentially a non-linear phenomenum, and cannot believe very strongly in this type of theory.

Carr came back once more to the subject of Fritz John's floating bodies theory. He has better test facilities than they have at Berkeley, and I had the impression that he would have liked to try his hand at the experiments. Carr told me that he thought the Bureau of Harbors and Docks would be interested in this work; however, he thought they would be more interested if we extended the theory somewhat. John's theory applies (at least in the cases for which we have made calculations) only to bodies which are broad in the direction of wave travel as compared with the depth of the water, and this, of course, is a serious limitation on the practical application of the results. It would be better to extend the theory to a system of floating bodies attached to each other, say, by smooth hinges. In this way cases could be treated theoretically which approximate better possible designs for practical purposes. I think we can do this. I should also talk with the proper people in the Bureau of Harbors and Docks on some occasion.

I returned at 11:00 to Pasadena, in order to talk with Bohnenblust (Professor of Mathematics). I wanted to talk to him about non-linear vibrations, since that has been one of his interests in recent years. I told him about my book, of course, and we talked about a few problems in the field. Most of the time, though, we spent talking about partial differential equations and about the seminar we had this year on the subject. Afterwards I had lunch with Bohnenblust and DePrima.

In the afternoon I talked with Lindwall (Head of the Engineering Division, California Institute of Technology). We talked first generally, and then about computing machines. Lindwall took me to see their electric analogue computer, and explained something of how it works and what problems they can handle. These are quite varied; first, linear equations; second, non-linear electric circuits; third, continuous structures, both steadystate and transient.

I next went to see Hans Liepmann (Aeronautics Department). We talked a little bit about non-linear vibrations and about what Friedrichs and Gardner are doing. Oddly enough, Liepmann thinks that they would be interested in certain results in non-linear vibrations, in the hope of using them for certain things in turbulence. I expressed some skepticism, since so far the only systems we have treated in non-linear vibrations are the simplest kind of systems with one degree of freedom, while in the turbulence problem the systems are obviously extremely complex. Liepmann was not disturbed by the skepticism. He thinks there still might be something in it. Liepmann will probably visit us in New York within a few weeks.

Saturday, June 11: I left Pasadena for Chicago by train.

Monday, June 13: I arrived in Chicago at noon, and left at once for Ann Arbor to attend this year's Symposium on Applied Mathematics of the American Mathematical Society. The subject this year was elasticity, and the meetings were held jointly with the American Society of Mechanical Engineers.

Tuesday, June 14, through Thursday, June 16: I attended the meetings and conferences at Ann Arbor, together with Dressler and Friedrichs, who had come from New York to attend the meetings. On Tuesday morning I read a paper myself on the subject, "Strengthening a thin plate against buckling by inserting a wedge." There was a lively discussion afterwards, as people seem to have been amused by the idea. Afterwards it was suggested by several people that I should patent the idea. Someone also suggested that the Navy might be interested from the standpoint of design of bulkheads for submarines. Perhaps I should speak to

someone in the appropriate Navy bureau about it.

As usual, I found the meetings stimulating but exhausting.

Friday, June 17: Friedrichs and I went to Pittsburgh from Detroit in order to talk with Pugh (Carnegie Institute of Technology) about the work on shaped charges. Pugh and his group were very cordial, and we had two or three hours of lively and stimulating talk. Friedrichs will doubtlessly write his comments on the technical aspects; for my part, I thought the conference a more than usually useful one for both participating groups.

Returned home to New York that night -- very glad too, after an absence of three weeks.

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